among other things, as well as to give everybody an electric light.

"Probably you think it very strange that I should show you the inefficiency of electric transmission of energy, and then make this very bold assertion. Well, the fact is that the ordinary electrical machines in use have not been constructed with a view to economy. They have been constructed to show that brilliant lights and considerable power may be produced from small machines. They have, at a comparatively small cost, attracted attention to the fact that electricity is an important agency. In so far they have done well; but on the other hand they gave rise to the well-known assumption that 50 per cent. of the mechanical power given to the generator was the maximum amount which could be taken from the motor. The true solution of the problem of transmission of power was, I believe, first given by Prof. Ayrton in his Sheffield British Association lecture. It had been supposed that to transmit the power of Niagara Falls to New York a copper cable of enormous thickness would be needed. Mr. Ayrton showed that the whole power might be transmitted by a fine copper wire, if it could only be sufficiently well insulated. He also showed that instead of a limiting efficiency of 50 per cent., the one thing preventing our receiving the whole of our power, is the mechanical friction which occurs in the machines. He showed, in fact, how to get rid of electrical friction. I will briefly give you our reasons. A machine at Niagara receives mechanical power, and generates electricity. Call this the generator, and remember that Wall-sheet III. teaches us that the mechanical power is proportional to the electromotive force produced in the generator, multiplied into the current which is actually allowed to flow. Let there be wires to another electric machine in New York, which will receive electricity, and give out mechanical work, as this machine does here. Now I showed you a little while ago that this machine, which may be called the motor, produces a back electromotive force, and the mechanical power given out is proportional to the back electromotive force multiplied into the current. The current, which is of course the same at Niagara as at New York, is proportional to the difference of the two electromotive forces, and the heat wasted is proportional to the square of the current. You see then, from Wall-sheet III., that we have the simple proportion—power utilised is to power wasted, as the back electromotive force of the motor is to the difference between electromotive forces of generator and motor. This reason is very shortly and yet very exactly given in Wall-sheet IV., a printed copy of which you all hold in your hands."

WALL-SHEET IV.

Let electromotive force of generator be E; of motor F. Let total resistance of circuit be R. Then if we call P the horse-power received by the generator at Niagara. Q the horse-power given out by motor at New York, that is, utilised. H the horse-power wasted as heat in machines and circuit. C the current flowing through the circuit.

$$C = \frac{E - F}{R}$$

$$P = \frac{E(E - F)}{746 R}$$

$$Q = \frac{F(E - F)}{746 R}$$

$$H = \frac{(E - F)^2}{746 R}$$

$$Q : H :: F : E - F.$$

"To put it more shortly still, the power wasted is proportional to the square of the current flowing, whereas the power utilised is proportional to the current, and also to the electromotive force of the motor. The greater, then, we make the electromotive forces, the less is the loss of power in the whole operation. Perhaps you will see this better from the water analogy. A small quantity of water flowing through a water-main may convey a large amount of energy, if it only has sufficient head. The frictional loss of power is independent of the head, but depends very much on the quantity of water. In the model before you is the water analogy. Here is a reservoir, which I shall call A, kept filled with water by a steam pump, which draws the water from the sea-level, which I shall call K. Water flows from reservoir A to distant reservoir B, where it drives a turbine giving out work due to its head B K. The current from A to B, through the communicating pipe, is the same always, so long as A and B are at the same difference of level, and therefore the frictional loss of energy is always the same, whereas the work utilised from B, by driving the turbine, increases proportionally to the height of B above sea-level. The result, then, to which the above laws led us was that for the future development of the transmission and distribution of electric energy it will be necessary to use electric machines of great electromotive force. Indeed so important must this principle become that we believe there is a future in this direction for the employment of even plate electrical machines, such as that of Holtz."

Then followed a discussion of methods of obtaining great electromotive force. Mr. Perry's own ways of carrying out these ideas are shown in his own dynamo-machine, which is large, has great speed, has no iron in its movable part, and has a commutator of small frictional resistance. Electric lighting and heating, telephones, and electric railways of the future were all spoken of as illustrations of the transmission of energy by electrical means, and as such they must be governed by the above principle.

It was then shown experimentally that electrical energy may be stored up in considerable quantities in an available form for future use, and the bearing of this fact on the future utilisation of great but variable natural sources of power, such as the wind and tide, was dwelt upon.

The remainder of the lecture was devoted to the importance of the principle of recurrent effects; one illustration was given as follows:—"If I very much alter the magnetic field in this telephone, by bringing a powerful magnet near it, with great care in listening I hear the faintest sigh, due to the diaphragm settling itself into a new position, its vibrations dying away as it does so; and if I brought a small magnet near, I should hear nothing. And yet the change of magnetism which produces the loud telephonic effects which we listen to is almost infinitely smaller. Why is this? It is due to the rapid recurrence of the effects. Now you are all aware of the importance of the telephone as a method of communication; I believe that a much greater importance is in store for it as a laboratory appliance."

The photophone and the method by means of which Messrs. Ayrton and Perry determined the index of refraction of ebonite, finding its square to be roughly the same as the mean value of its measured specific inductive capacities: the use of a powerful sub marine source of musical sound as a coast-warning, which might be heard in a ship well above all other sounds, and the experiments which have been made by the lecturer and his colleague in this direction: these and other matters were discussed as examples of the use of the principle of recurrent effects. The lecture concluded by an account illustrated by experiments of Mr. Edward Bright's method of de-electrifying woollen yarn, and of Messrs. Ayrton and Perry's plan for seeing by electricity what is occurring at a distant place. A selenium cell moving over an image at, say, York, gave corresponding light and shade to corresponding parts of a screen at, say, London. Mr. Perry's York image was very simple, being a series of black, grey, and white squares, which were faithfully reproduced on the distant screen.

MECHANICAL RESEARCH

IT will be remembered that some time ago the Institution of Mechanical Engineers appointed a Committee to examine into three selected questions of research in matters pertaining to their profession. These researches are still in progress, but preliminary reports have been issued by the Committee, of which we propose to give a brief account.

The Hardening and Tempering of Steel.—One or two letters on this subject have lately appeared in our columns, and allusion has been made to the report by Mr. Wm. Anderson, presented to the Committee who were appointed specially to investigate this difficult question. Mr. Anderson's report, which contains much useful information in a comparatively small compass, is itself too long for our pages. We therefore give the following *résumé* of the question, taking Mr. Anderson's report as our basis:—

Whilst the theory of this subject is in a very vague and uncertain condition, the facts are exceedingly well known, and are daily applied in almost every department of arts and manufactures. Wherever steel tools are used it is necessary that they should be hardened and tempered; since the ordinary tool-steel, as supplied chiefly from Sheffield, is too soft for cutting and abrading purposes. It is known however that if a piece of this steel be heated, and then suddenly cooled (generally in a bath of water or oil) it becomes much harder, not only on the surface, but throughout, provided its thickness be not excessive. The greater the range of this cooling (in other words, the difference between the temperatures of the steel and the bath at the first moment of "quenching") the more intense is the hardening, but at the same time the greater the brittleness of the piece. Hence it is always desirable that the range of cooling should be as small as is consistent with the steel acquiring that degree of hardness which is essential for the work it has to do. This condition is secured by the further operation of tempering. In this process the steel is first hardened to excess by rapid cooling, then re-heated with great care to a certain temperature corresponding to the purpose for which it is intended, and then quenched again from that temperature. The particular point at which to stop the re-heating is recognised by one particular hue in what are called "the colours of tempering," *i.e.* a fixed range of colours, commencing with pale yellow and ending with dark blue, which the steel is always seen to assume in succession as its temperature gradually rises. Thus, if the article in question be a sword it is heated to a bright blue; if it be a cold chisel it is stopped at a brownish orange.

The various attempts to explain these singular facts (at least on the part of French and English metallurgists) are set forth in the Committee's Report. In the first place it seems now to be generally held that pure steel is a compound of iron and carbon only, and that these two elements exist, not in a state of chemical combination (forming some definite carburet of iron), but of intimate mechanical mixture, such as chemists call by the name of "solution." The question next arises, What is the exact condition of each of these independent elements. In the case of very soft, or "grey," cast-iron, it is known that the carbon is not wholly in solution, but occurs partly in molecules of pure graphite. Following this hint M. Jullien has advanced the theory that molten cast-iron, or molten steel, is a solution of liquid carbon in liquid iron; that under slow cooling part of the carbon separates as graphite, while the remainder continues in solution; but that with rapid cooling this separation does not take place, and the whole of the carbon crystallises, forming, when cool, a "solution" of crystallised carbon in amorphous iron. This view of the difference between hard and soft cast-iron, or hard and soft steel, is accepted by Caron, Åkerman, and others, partly on the ground that hardened steel dissolves completely in hydrochloric acid, while the same steel, after annealing, will leave a residue of insoluble carbon. Jullien, however, goes beyond this, and would explain the whole phenomena of hardening on the same principle. He holds that carbon liquefies in presence of red-hot iron, and is absorbed by it; that if the mixture is cooled slowly the carbon remains amorphous, but if cooled quickly the carbon crystallises in the diamond form; and thus hard steel is iron set in a matrix of diamond. This theory, though ingeniously supported, labours under the difficulty that the liquefaction of carbon has never been otherwise achieved; and also that it gives no explanation whatever of the phenomena of tempering, especially the characteristic colours. On the other hand, Barba and Åkerman hold that the hardening of steel is due to the severe compression produced in the outer layers by the contraction in rapid cooling; this compression at once retaining a greater proportion of carbon in solution, and rendering the whole mass more physically dense and compact. But the Report points out that the outside layers, which are the hardest, are brought into a state of tension, not compression, owing to their inability to contract over the hotter mass inside.

For these reasons the Committee have rejected both these theories, and propose one of their own, due apparently to Mr. William Anderson, but suggested by the experiments of Edison on platinum wire, an account of which appeared in NATURE, vol. xx. p. 545. They refer to the generally-accepted fact that ordinary steel contains a certain proportion of occluded gases (consisting, according to Müller, of hydrogen, nitrogen, and carbonic oxide). They suggest that the application of heat causes these gases to be expelled through minute fissures which open in the steel, as they opened, according to Edison, in the platinum wire observed by him; and that sudden cooling prevents the re-absorption of what has been expelled, perhaps actually tends to expel the remainder. By the loss of these gases the metal becomes denser and harder than before. If the metal be now expanded by gentle heating, the fissures open, re-absorption begins; and the various changes which the surface undergoes during this process are marked by the succession of colours which are characteristic of tempering.[1] The Committee propose to make a series of experiments to test the truth of this theory, which is certainly ingenious, and if confirmed would go far to remove the difficulties which beset this important subject.

Form of Riveted Joints.—The second Report is written by Prof. W. C. Unwin, of Cooper's Hill, and is on "the best form of riveted joints to resist strain in iron or steel, or in combination." It may be well to explain that by a "riveted joint" is meant the mode of fastening together the strips, or plates, out of which boilers, tanks, girders, and other structures in wrought iron or steel are built up. This mode in its simplest form is as follows:—A row of holes is punched or drilled along the edge of the two plates to be united, and these edges are then made to lap over each other so that each hole in one plate comes fair with a hole in the other. A red-hot rivet (that is a pin with a rounded head) is then passed through both holes, and the end is flattened down by hammering or pressure, so as to form a second head. The two plates are thus pinned together by the rivets, and so long as these remain entire they cannot be separated without tearing across. Such a joint is called a "single-riveted lap-joint." It is obvious that the plate must be greatly weakened by the piercing of the holes; and as a matter of fact it appears that such joints cannot be arranged to give more than about half the strength of the solid plate. To increase this "proportion of strength," as it is termed, the rivets are sometimes arranged in two or three rows, and of course more widely spaced in each row; or the edges of the two plates are simply brought up against each other and secured by either one or two "cover-strips" fitting over and riveted to both.

Each of these forms demands a separate investigation in order to fix its design. Thus a single-riveted lap-joint under a tensile stress may fail in any one of the following ways: (1) the rivet may cut into the plate, enlarging and injuring the hole; (2) the plate may cut into the rivet, and finally shear it off; (3) the part of the plate between the rivet and the edge may break through, allowing the rivet to come away from the plate; (4) the plate may simply tear across along the line of rivets. It is clear that in a perfect joint the dimensions must be such that the resistance to each of these modes of fracture should be the same, and should have its greatest possible value. This could be easily arranged if the absolute resistance of the material to these various forms of stress were accurately known. But the values of these resistances are of course very different for steel and for iron; they also vary considerably, whether in steel or iron, according to the quality, and to some extent according to the thickness. Hence experiments on these values become absolutely necessary before any correct design can be made out.

The course which such experiments should take is fairly sketched out by the author of the Report now before us. A good and uniform quality of iron or steel, as the case may be, should first be selected, both for plates and rivets. The resistance of this material to the various forms of stress should then be carefully ascertained by experiments made both with simple bars or plates, and also with actual riveted joints, so designed that they shall be certain to fail in one particular way. These constants once settled, it is easy to calculate for any description of joint the dimensions which will give the highest proportion of strength. A joint should then, and not till then, be prepared, having exactly these dimensions, and a few others having dimensions varying slightly from these in each direction. If, on testing, the first joint proves to gives the highest breaking strain of the set, the correctness of the whole investigation will be established.

Unfortunately the method thus sketched out has not hitherto been adopted. The immense practical importance of the subject (for the money expended yearly on riveted structures may be counted by millions) has indeed brought forward a host of experimenters; and the mere classification and abstracting of their results occupies no less than sixty octavo pages of this Report. But almost without exception they seem to have begun at the wrong end, *i.e.* they started with making a riveted joint of what they chose to consider to be the best design, and then pulled it asunder. In addition, scarcely any of their experiments have been made with the care and accuracy, or on the scale, which the subject demanded. In fact it is not going too far to say that 90 per cent. of these experiments are only injurious, as cumbering the ground, and that the remaining 10 per cent., if useful, are very imperfect. One instance of the evil done by such means will suffice. Sir Wm. Fairbairn, to whom is due the credit of being the earliest labourer in this field, experimented on certain single- and double-riveted joints, and found that the "proportion of strength" in the case of the former was 56 per cent., and in the latter 70 per cent. of the solid plate. These figures, which of course applied only to the particular designs tested, have been repeated in almost all manuals of engineering as if they were universally true; disregarding the obvious fact that a double-riveted joint could be made just as weak as a single-riveted one, by simply spacing the holes in the outside row at the same distance.

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[1] The Report supposes that the colours of tempering are due to diffraction, not to interference: this does not seem to be in accordance with the facts, but it also does not seem to be absolutely required by the explanation.

The Committee wisely determined to throw aside the voluminous labours of their predecessors, and begin *de novo* a connected series of experiments, based on the true and scientific method described above. We cannot find space to consider the many collateral points with which these experiments will have to deal, much less to give any account of the results which they are to supersede. These, as embodied in this Report, will remain a singular instance of the lamentable waste of money so continually incurred in engineering experiments. There can be little doubt that less than a tenth of this money, if applied on the scientific and proper method, would have set the whole question long ago at rest, and would now be saving the world, through increased economy of construction, many hundreds per annum for every pound so expended.

Friction.—The last of the three subjects under consideration is that of Friction at High Velocities, the Report on which has been prepared by Prof. Kennedy, of University College, London. This subject offers a curious instance of the influence exercised by a distinguished experimenter, and how his conclusions are pushed, by those who blindly follow his guidance, much further than he himself would attempt to go. About fifty years ago the late General Morin made an important series of experiments, from which the well-known "Laws of Friction" were deduced. One of these laws is that the friction between solid bodies in motion, or dynamical friction, is independent of the velocity. It was overlooked, by those who announced this law, that the experiments were only conducted with certain substances under small pressures and at moderate speeds. General Morin himself, in an interesting letter published in the present Report, expressly states that he had himself always regarded his results, "not as mathematical laws, but as close approximations to the truth *within the limits of the data of the experiments themselves.*" Unfortunately others did not imitate this caution: they asserted everywhere that the law was universal, and by many it is asserted to be so still.

That it is not universal has however been sufficiently proved. At the time of the launch of the *Great Eastern* the late Mr. Froude showed, by experiments on a large scale, that the friction of a vessel on the launching-ways decreased rapidly as the velocity increased. In 1851 Poirée and Bochet showed that the coefficient of friction of railway wheels sliding on rails diminished very rapidly with increase of speed (between limits of 900 and 3600 feet per minute). Recently Capt. Douglas Galton and Mr. Westinghouse made a long series of experiments on the friction of railway-brakes (cast-iron blocks on steel tyres), and their results showed a marked decrease of friction, with increase of speed, within the very large range of 400 to 5300 feet per minute. Prof. Kimball has made experiments at much lower speeds (about 1 to 100 feet per minute), both with pieces of wood and with wrought-iron spindles in cast-iron bearings; and he also finds a rapid decrease of friction with increase of speed. At the lowest possible speeds (0·012 to 0·6 feet per minute) Prof. Fleeming Jenkin finds a similar decrease, pointing to the supposition that the change from statical to dynamical friction is not sudden, but continuous. Lastly, Prof R. H. Thurston has made an elaborate set of experiments on the frictional resistance of lubricated bearings. He arrives at the conclusion that for cool and well-lubricated bearings the coefficient of friction decreases up to a speed of about 100 feet per minute, and afterwards increases with the speed approximately as its fifth root. The details of these experiments do not seem to have been published, so that it is not certain how far this curious result may be taken to hold.

It will be seen that none of these various experiments confirm the universal law deduced from Morin's results, viz. that dynamical friction is independent of velocity. On the contrary, it may be taken as proved for *unlubricated* surfaces (such as railway brakes) that the coefficient of friction diminishes rapidly with increase of velocity; although the exact law of variation and its relation to the pressure on the surfaces is not fully determined. With *lubricated* surfaces the same fact may be assumed to be true at speeds up to 100 feet per minute; but above this, if we accept Prof. Thurston's results, the result is the opposite. It seems clear that the question is ripe for further investigation, which might take the form, first of repeating and extending Thurston's experiments with lubricants, and secondly of ascertaining the law of variation with unlubricated surfaces more exactly than could be done by the aid of the experiments hitherto carried out.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE

CAMBRIDGE.—At the Downing College Examination in June, 1881, one Foundation Scholarship of the annual value of 80*l.* will be thrown open to all members of the University who have not kept more than six terms. The subjects of this Examination will be Chemistry (Theoretical and Practical), Physics (Heat, Electricity, and Magnetism), Comparative Anatomy, Physiology, and Botany. The Examinations for Minor Scholarships, which are open to all persons who have not entered at any college in the University or who have not resided one entire term in any such college, will be held in Downing College on Tuesday, May 31, and three following days. Further information will be given by Mr. J. Perkins or by the Rev. J. C. Saunders, tutors of the College.

AT a special meeting of the Fellows of Gonville and Caius College, held on the 30th ult., Dr. Paget, F.R.S., Regius Professor of Physic in the University, and Mr. Pattison Muir, Hon. M.A. (Cantab.), were elected Fellows of the Society. Dr. Paget was formerly a Fellow of Caius College.

OXFORD.—In addition to the courses of lectures in Natural Science enumerated last week, the following courses will be held during this term in the University Museum:—Prof. Price will lecture on physical optics, and Prof. Westwood will lecture on the orders of the Arthropoda. In the absence of Prof. Rolleston, who is abroad on account of ill-health, Mr. Jackson will form classes for general catechetical instruction, while classes will be formed by Mr. Robertson for practical microscopy, and by Mr. Thomas for the study of the developing chick.

At the Botanical Gardens Prof. Lawson will lecture on elementary botany (development), and will continue his course on the dissection of plants.

In the Geological Department under Prof. Prestwich, lectures will be given on some of the secondary and quaternary strata. The Professor will have excursions to inspect the sections of the several formations around Oxford, commencing on Saturday, April 30, and to be continued through May. On each preceding Friday he will lecture on the subject of the following Saturday's excursion, or on some other subject of which notice will be previously given. Notice will also be given in the Gazette of the preceding week, and in the Museum, of the places to be visited, hours of meeting, &c.

In a congregation holden on Tuesday, May 3, the proposal to allow selected candidates for the Indian Civil Service to obtain the B.A. degree after two years' residence, was thrown out. An amendment to excuse selected candidates from responsions only was carried by 63 votes to 49.

THE scheme for the establishment of a University College in Liverpool is now almost matured, and it is expected that the College will open for its first session in October next. The donations have reached the sum of 100,000*l.*, and the task of drafting a constitution for the College is now being performed by a special committee. The Earl of Derby has accepted the office of president, the vice-presidents being Mr. Christopher Bushell and Mr. William Rathbone, M.P.

SCIENTIFIC SERIALS

Journal de Physique, April.—Theory of machines with alternating currents, by M. Joubert.—On radiophony (second memoir), by M. Mercadier.—Application of Talbot's fringes to determination of the refractive indices of liquids, by M. Hurion.—Apparatus for projecting images at any distance with a variable enlargement, by M. Crova.—Strong and constant voltaic pile, furnishing residues capable of regeneration by electrolysis, by M. Reynier.